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Peer review report 3 on DAILY DISAGGREGATION OF SIMULATED MONTHLY FLOWS USING DIFFERENT RAINFALL DATASETS IN SOUTHERN AFRICA

Original Submission

Recommendation

Major Revision

Comments to the author

"Daily disaggregation of simulated monthly flows using different rainfall datasets in southern Africa", by D.A. Hughes and A. Slaughter

In this manuscript, the authors discuss the implications of using different rainfall data sets for streamflow modeling in 19 river basins located in Southern Africa. The main particularity of this study is that the hydrological modeling itself is carried out at a monthly time scale using the Pitman hydrological model and the simulated monthly streamflow is subsequently disaggregated to daily values. The study is overall appropriate for publication in EJRH. However, the manuscript needs substantial improvements before it can be considered for publication.

My main criticism deals with the main conclusions of this study: "α. . .it is recommended that all studies should begin with a thorough examination of all available data sources" (l. 493, ff.). In my opinion, this is an essential part of any scientific endeavor in general, including the field of hydrology. Any hydrologic modeler knows this! The second main conclusion is also well known, which is that daily streamflow simulations will never be better than monthly ones (l. 505 ff.), as the temporal variability is greatly reduced when moving from daily to monthly flows.

However, the authors address the problem of hydrologic modeling in data poor environments (quantity and quality wise). My recommendation is to provide more details on the given data sources. Are the given datasets closing the water balance at all? What are the runoff ratios of the single river basins? Are these realistic or are they so unrealistic that any modeling initiative is doomed to failure, which is suggested by the hydrographs seen in Figure 5 and 6 and by the discussion given in paragraph

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starting at line 341. I suggest to expand section 3.1 and 3.2 (of the results section, not the method section) for this purpose. I also suggest to give more details on the river basins in Table 1 in a similar fashion as has been done by Evin et. al., WRR 2014, DOI:10.1002/2013WR014185 in the respective Table 1. I find quantitative catchment characteristics more informative than the description given in Table 1 in this manuscript.

Some additional points

Methods: The author have to give at least a brief description of the employed hydrological model (l. 122, ff.). I would like to know at least what kind of input it 1does require, how many free parameters it has, and which processes are included. The same is true for the daily disaggregation method. The description given in lines 124 and following are too vague and need to be improved. This is an even more important issue as I could not find the given publication of the method (i.e., Slaughter 2014). Equations given in Figure 1 should also be mentioned in the main text or an appendix.

Introduction: The author should mention alternative hydrological modeling approaches. For example, there are disaggregation approaches for gridded precipitation fields available such as Thober et. al., WRR 2014, DOI:10.1002/2014WR015930. Is it expected that an approach that first disaggregates the monthly precipitation field and then conducts the hydrologic modeling at the daily scale lead to a higher performance than the procedure suggested in this smanuscript?

Results: Line 341 ff.: The authors should consider using objective streamflow signatures (see for example Euser et. al., HESS 2013, doi:10.5194/hess-17-1893-2013).

Line 343 ff.: Why is a value of 0.99 a quite high value with respect to 0.98? I would consider these values very similar.

Line 367 ff: "Even with. . ." The author need to be circumspect with statements like these. After all, the model structure could miss a process required to simulate the streamflow at this location and time period adequately. This has to be discussed at least.

Language: The manuscript is not carefully proof-read. For example, the meth- ods and results part are both enumerated with 3. There are various typos in the text that I do not highlight here as I expect a substantial revision of the text. For example, the author frequently use the phrase "Figure/Table suggests. . ." (l. 279). To my understanding, this is not possible. The results shown in the Figure are able to suggest something, but not the Figure itself.

Overall, my recommendation is major revision. I encourage the authors to focus more on the quality of the data in a revised version of the manuscript and how they could achieve the best possible performance out of it.

First Revision

Recommendation

Minor Revision

Comments to the author

"Daily disaggregation of simulated monthly flows using different rainfall datasets in southern Africa", by D.A. Hughes and A. Slaughter

The authors substantially improved the manuscript with this revision. In particular, they expanded the descriptions of the Pitman model (l.∼103ff.). This increases the readability of the manuscript as the basic hydrologic processes involved in the modeling are now mentioned as well as the dimensionality of the parameter space. Also the publication of the disaggregation method (i.e., Slaughter 2015) is now available wich was not the case during the first revision round. More importantly, the discussion on the mismatches between rainfall variations and flow responses has been expanded (l.∼396ff.) and alternative approaches are now mentioned (l.∼448ff.). The influence of the API parameters has also been clarified throughout the manuscript (l.∼370ff. and l.∼480ff.).

Overall, I find most of my comments of the first review well addressed. I only have one minor comment left that needs to be addressed before this manuscript can be published, which is the following:

I agree with the authors that the equations and steps presented in Figure~1 should not be repeated in the manuscript to unnecessarily lengthen it. However, I do not see any information presented in this Figure that requires the format of the Figure. This Figure does not contain a graph or a flowchart. This Figure is only an enumeration of processing steps which are text. This text should, in my opinion, be incorporated in the methods part to further enhance the readability of the manuscript and the Figure could be removed. I apologize if my earlier comment on this aspect has not been clear enough.

Anonymous reviewer 3

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